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REMARKS
ON THE
LEADING PROOFS
OFFERED IN FAVOUR OF THE
FRANKLINIAN SYSTEM
OF
ELECTRICITY.

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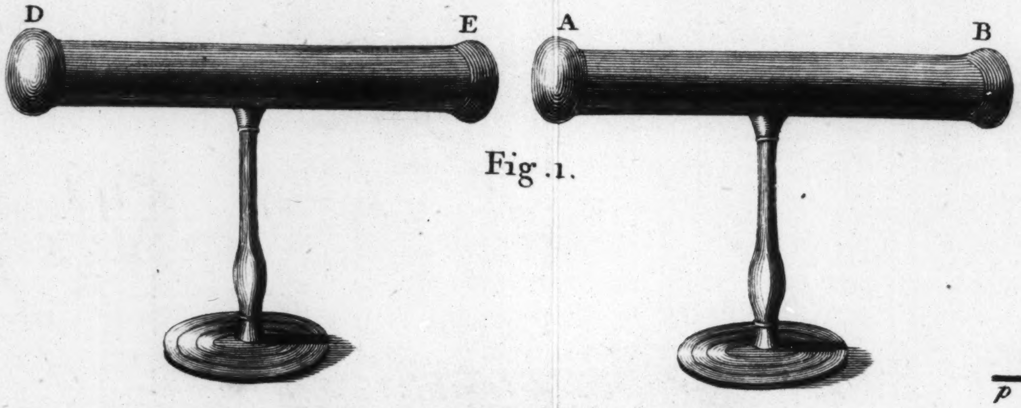


Fig. 1.

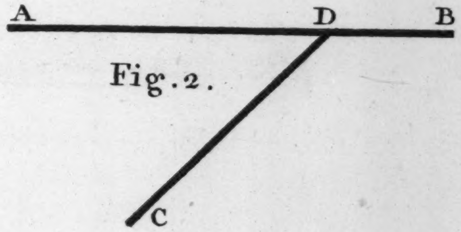
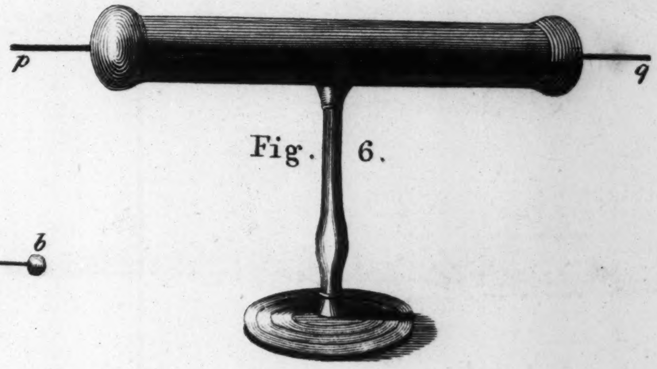


Fig. 2.



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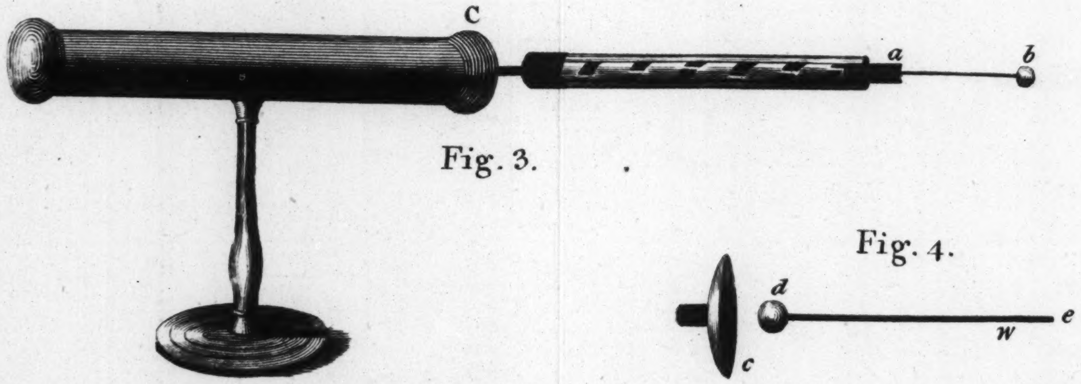

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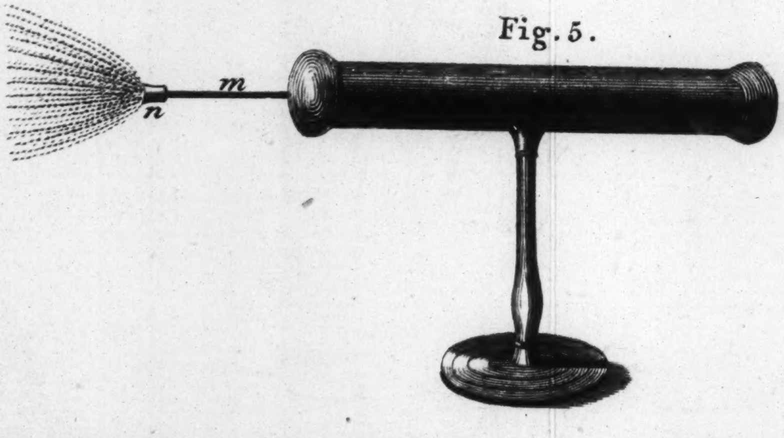

Fig. 5.

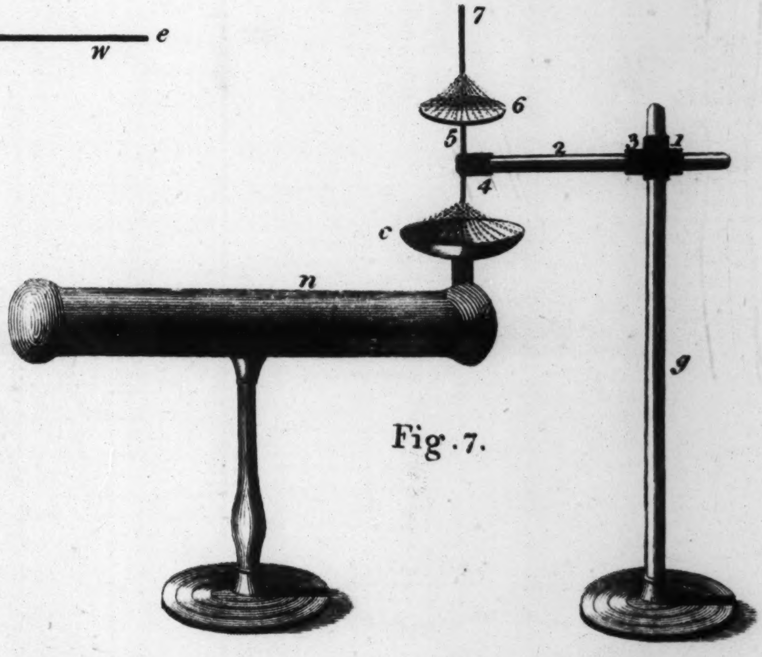

Fig. 7.

J. Lodge sc.

REMARKS
ON THE
LEADING PROOFS
OFFERED IN FAVOUR OF THE
FRANKLINIAN SYSTEM
OF
ELECTRICITY;
WITH EXPERIMENTS
To ſhew the Direction of the Electric Effluvia,
viſibly paſſing from what has been termed
NEGATIVELY ELECTRIFIED BODIES.

By the Rev. JOHN LYON, Dover, Kent.

> Lorſqu'un Syſteme eſt contraire à la Nature, il eſt bien rare qu'il n'ait pluſieurs côtés foibles, & que par conſéquence, il ne ſoit ſuſceptible d'être attaqué de plus d'une manière : c'eſt le cas de ceux dont je parle.
>
> Lettres Phyſiques & Morales. Tom. 1. p. 246. II. Partie.
> Par. J. A. De Luc.

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M.DCC.XCI.

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THE reader will learn from the motto on the title page, that I do not think the Franklinian ſyſtem of Electricity, founded on the ſimple operations of nature: and I as much doubt, whether it can be reconciled with the appearances which our experiments exhibit; and I farther conſider it as a mere hypothetical ſuppoſition, which has been the occaſion of introducing as great apparent inconſiſtencies in the works of our writers on this ſubject, as are to be found in the old ſchool of philoſophy.

The reader will find in the following ſheets, a few of theſe apparent inconſiſtencies very briefly related and he may reſt aſſured, that if my deſign had been to increaſe the number of pages, this pamphlet might have been ſwelled with ſimilar extracts, to a very large volume; but what I have collected, and compared, I judge will be ſufficient to ſhew, that there is ſome error, either in theory or in practice.

As I am ſenſible that a little dampneſs on the ſpiral tube, and the inſulating parts of the apparatus, mentioned in the following ſheets, may prevent the appearances, by leading off the electric effluvia unperceived; care muſt be taken, that the ſpiral tube be firſt dried by the fire, and the inſulating parts well rubbed; and I have alſo to add, that in order to ſucceed well with the experiments, the excitation of the cylinder ought to be ſtrong.

If the experiments are made as they ought to be, I truſt the appearances produced, will determine the direction of the electric effluvia to be from what is called a negatively electrified conductor; and, if I have erred in my obſervations and remarks, I ſhall be obliged to any electrician, who will point out the errors to me.

It has been hinted to me, that in philoſophic, and ſcientific works, the principles, and the terms of art made uſe of, ought to be explained for the benefit of the young and unexperienced; or they cannot, without a conſiderable expence, be able to judge of points which are diſputed; I ſhall therefore, for the ſake of ſuch perſons add, that electricians have divided into two claſſes, all known ſubſtances; and they diſtinguiſh them by the names of electrics, and conductors.

1. Electrics, or non-conductors, are ſuppoſed to be ſuch bodies as do not admit the eleſtric effluvia over their ſurfaces, or through their pores.

No ſuch electric has been proved to exiſt.

2. Dry air is placed firſt, in the claſs of electrics.

This ought to be particularly noticed in reading the following ſheets.

3. All inſulated conducting bodies, connected with the prime conductor, are ſaid to be poſitively electrified, while the cylinder is turned.

4. When the rubber, or cuſhion, is inſulated, then inſulated bodies connected with it are negatively electrified, or, they are ſaid to have leſs than their natural quantity of the electric effluvia.

5. A lucid pencil of rays ſeen on a pointed wire in the prime conductor, is one of the infallible ſigns of the fluids iſſuing from it.

6. The direction of the electric fluid is ſaid to be from the inſulated cuſhion, and every conducting ſubſtance connected with it, to the cylinder; and when they have no communication with the floor, they are ſaid to be deprived of their natural quantity of the electric effluvia, by turning the cylinder.

8. The lucid pencil of rays ſeen on a pointed wire, when preſented near the broad ſurface of any negatively electrified body, is ſaid to be one of the infallible ſigns of the electric effluvia iſſuing from it.

This ought not to be taken for granted, for the pencil is very different from one iſſuing from

from a point in the conductor poſitively electrified.

If the foregoing rules be attended to, they will be found ſufficient to enable a perſon to judge of the following experiments and remarks; and if they want to enter farther into the ſubject, they may conſult the various authors who have written on electricity, for information.

CHAPTER I.

Containing the Obſervations of different Authors, on the permeability of Glaſs to the Electric Effluvia; and on poſitive and negative Electricity, with Brief Remarks on them.

AMONG the ſeveral objects of philoſophical inveſtigation, which have for ſome time paſt attracted the attention of the curious, electricity has been one of the firſt claſs; and in the courſe of about fifty years, an almoſt infinite variety of particular, unconnected experiments, have been aſſembling from all quarters; but, excepting in a few inſtances, to very little purpoſe.

When the inquiſitive and attentive mind, ſurveys with an impartial eye, what has been produced by the labours of ſo many experimental inquirers, in this branch of natural philoſophy; (as well as in ſome others of modern reſearch) it appears little better than a rude indigeſted heap, and we ſeem as far diſtant as ever, from any juſt theory of electricity. This prevailing confuſion, ſeems to be owing to the unſcientifick manner in which the experiments appear to have been made; and our ſlow advances to clear, comprehenſive, and determinate conceptions, on the true cauſe of electric phenomena, may be aſcribed to a very ſtrong attachment to the hypotheſis contrived by Doctor Franklin, for explaining the intricate effects produced by a charged jar; as every attempt muſt be ſtrained by the Frankliniſts, into conſonance with the theory of their maſter.

With this biaſs upon the judgment, which attends, or rather precedes, the philoſopher in his inquiry, how can he either clearly diſcern, or fairly interpret, what nature teaches? Or, how can he expect to eſtabliſh a ſyſtem, agreeable to any general law, by which the electric effluvia may be fairly proved to act; and by which, every difficulty may be ſolved, without inconſiſtency?

The doctor's fundamental principle is the [a] impermeability of glaſs to the electric effluvia, which he conſiders as abſolutely neceſſary to the forming of the charge; but a little reflection might have taught him, *that if a fluid be poured into any veſſel, faſter than it can run out of it, that veſſel will fill.*

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[a] See Franklin's Letters, page 73, and 161.

But, beſides the uncertainty, and improbability of this principle, he is obliged to have recourſe to another, equally hypothetical, namely, that [b] glaſs has as much of the electric effluvia in its pores as it can hold; and that its natural quantity, can neither be increaſed or diminiſhed.

This hypotheſis is one of the main pillars of his ſyſtem; and, if this be taken away, the whole fabric will be ruined. If we admit this principle, a difficulty preſents itſelf at the very firſt ſtep.

If the electric fluid be elaſtic, is it eaſy to conceive how, in a line of particles, of an expanſive nature, the moſt diſtant from the point of preſſure, ſhould be removed from their place, while the intermediate particles continue immoveable, and without preſſing forward into the unreſiſting ſpace, evacuated by the repelled particles? Glaſs is ſuppoſed to be ſingularly attractive of the electric matter, and this attraction muſt increaſe the natural tendency of the electric particles, to expand in the ſuppoſed void parts, that they may be equally diffuſed through the whole ſubſtance of the glaſs.

There does not appear to have been any concluſive reaſon offered by electricians, from the internal ſtructure of glaſs, for the fixedneſs of the electric effluvia in its pores.

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[b] See Franklin's Letters, page 26.

Doctor Franklin ſays, "my [c] hypotheſis, "that the pores of glaſs were too ſmall in the "middle, to admit the electric particles to paſs "through, was *certainly wrong*." In order to confirm his theory, he made an experiment, by grinding away five ſixths of the thickneſs of the ſubſtance of one of his phials, and the bottle charged as well after the grinding as before. He then adds theſe remarkable words, "I am now as much as ever at a loſs "to know, *how*, or *where*, the quantity "of electric fluid on the poſitive ſide of the "glaſs is diſpoſed of;" for he had before determined the power [d] of giving a ſhock is in the *glaſs itſelf*.

Whoever attentively conſiders the reſult of the experiment to diſcover where the ſtrength of the Leyden phial lay, after decanting all the water from the charged jar, and pouring in freſh unelectrified water, cannot, I think, but ſuſpect, that what is called the charge, is ſome very different affection, or ſituation, of the electric matter, from what it is generally ſuppoſed to be.

With reſpect to the impermeability of glaſs to the electric effluvia, it is intirely hypothetical, neither is there a ſingle [e] determinate experiment to prove it; and the ſtrong-eſt advocates for it, have been frequently obliged to deny in practice, what they have taken for granted, and poſitively maintained in theory.

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[c] Dr. Franklin's Letters, page 321.

[d] The ſame, page 26.

[e] The ſame, page 27.

Since the evidence which experiments exhibit cannot always be evaded, it has been admitted as probable, that ſome *glaſs may be permeable*[f] in ſome ſmall degree when cold, by having a greater quantity of non-electric matter in its compoſition, as *all glaſs is found to be*, when warmed.

This confeſſion makes very much againſt the received theory; for it is well known, that warm glaſs charges better than cold; therefore glaſs, which is permeable to the electric effluvia, in a certain degree, is beſt for electric jars.

[g] Another writer on this ſubject tells us, it has been obſerved, that the *hardeſt and beſt vitrified glaſs is a very bad electric, it being ſometimes quite a conductor.*

[h] The Abbe Nollet found the glaſs of Saint Gobin and Cherburg (the *hardeſt*, the moſt *compact*, and beſt *vitrified*, of all French glaſs) was the moſt difficult to be electrized; whereas the cryſtal glaſs of England, and that of Bohemia, which are much ſofter, were the *beſt of all* for electrical experiments.

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[f] Doctor Prieſtley's Hiſtory of Electricity, 2d Edit. page 400.

[g] Cavallo's Complete Treatiſe, page 7.

[h] Doctor Prieſtley's Hiſtory of Electricity, page 189.

It

[i] It is further added, there is a ſort of glaſs, like that of which Florence flaſks are made, which on account of ſome *unvitrified particles in its ſubſtance*, is not capable of holding *any* the *leaſt charge*.

Is not this acknowledging, notwithſtanding all that is perſiſted in to the contrary in *theory*, *that in experiment* it has been found, that the *beſt vitrified*, and the *worſt vitrified*, and the *cold*, and the *warm glaſs*, are *all*, *more* or *leſs permeable*; and, may I not ſafely conclude, that all glaſs is ſo in *a dry ſtate*?[k]

The ſame inconſiſtency will be found in the experiments made to prove, that glaſs jars, or plates, when charging, or charged, have always one ſurface in a poſitive, and the other in a negative ſtate, at the ſame time.

[l] Mr. Wilſon holding a pane of glaſs a little warmed, upright upon a cake of wax, found, that by rubbing the ſurface of one ſide in the middle with his finger, that both ſides were poſitively electrified.

Though this appearance manifeſtly claſhed with the favourite ſyſtem, which ſays, that as much electric effluvia is repelled from one ſurface, as is added to the other, there was no difficulty in reconciling it to the principles of the theory; for we are told, it ought to be ſo, "because, if a pane of glaſs be rub-"bed in this poſition with the finger, the "ſurface acquires ſome of the electric effluvia "from it; and this repels an equal quantity "from the other ſide of the pane, where it "ſtands as an atmoſphere, and there not be-"ing any conducting ſubſtance to lead it off, "both ſides are found poſitively electrified."

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[i] Cavallo's Complete Treatiſe, 1ſt Edit. page 144.

[k] Lyon's Eſſays and Obſervations on Electricity, page 20, and further proofs, page 30.

[l] Doctor Prieſtley's Hiſtory, page 398.

Doctor Prieſtley, to ſave the credit of a favourite ſyſtem, endeavoured plauſibly to evade the force of Mr. Wilſon's experiment, without recollecting that he had allowed *all glaſs to be permeable when warm*: for here, in direct contradiction to his own conceſſion, he rejects a plain matter of fact, which proves what he admits; and through a fond predilection for a ſpecious theory, he no leſs unphiloſophically, than unfairly, has recourſe to a mere hypotheſis.

If the advocates for the Franklinian ſyſtem, can by any means get rid of a particular difficulty, they ſeem ſeldom to look any farther, or take time to conſider, how their ingenuity may affect the general principles of their theory.

I am ready to admit that the doctor's ſolution of this appearance may paſs upon paper with ſuch as read, to find arguments to ſupport ſyſtems; but ſuch as read and compare, will find one unlucky circumſtance attending it, which is, that this ſolution envelopes their important doctrine of poſitive and negative electricity, in doubt and uncertainty;

for if, as in the caſe before us, when one ſide ſhews the ſame ſign of poſitive electrification as the other, though it muſt be really negative, according to his own reaſoning, the *electric effluvia being driven*, he ſays, *out on the ſurface of the ſide oppoſed to the rubbed ſurface*; how ſhall we determine on the poſitive electrification of a glaſs ſurface, if the general law be ſubject to ſuch a groſs error?

If I miſtake not, the poſitive charge has conſtantly been ſuppoſed to reſide in the pores of the glaſs, and to be ſomething very different from an atmoſphere, if there be any truth in Doctor Franklin's theory, which I much doubt, notwithſtanding all the reaſoning which is produced in the ſubſequent part of the page laſt referred to, to involve a plain fact in difficulty and obſcurity.

[m] But again, Mr. Wilſon having a pane of glaſs with one ſide rough, and the other ſmooth, by rubbing them as mentioned before, he found both ſides negatively electrified.

This appearance cauſed no difficulty, as the ſame theory was to explain it, and the Doctor's idea appears to have been this; increaſe and diminution, are oppoſites, and muſt have oppoſite effects. In the preceding inſtance of the finger's ſupplying the rubbed ſurface with the electric effluvia, it is ſaid this drives out the laminæ of fluid on the oppoſite ſide; but when the finger receives the fluid by friction, as in the preſent inſtance, then the ſame laminæ retire inward.

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[m] Dr. Prieſtley's Hiſtory of Electricity, page 399.

This ſurely is fallacious reaſoning, it is drawing concluſions from ſounds, and not from the nature of things. If I admit for a moment, that the pores of glaſs in their natural ſtate, are ſo full of the electric effluvia that they cannot hold any more, then, in the inſtances I have mentioned, the terminating laminæ are acted upon, and act upon each other, by the intermediate laminæ, and all are at reſt through the whole maſs, by the equal action of the ſeveral parts. Now if I ſuppoſe the counteracting force of the lamina A, at one ſide be taken away, which is the ſame in effect as if the other had been abſolutely increaſed, then the lamina B, the next to A, being impelled by the fluid behind, muſt move forwards to the part left by A, and in this manner the whole maſs, by a general expanſion, will be rarified, and the glaſs will become, as it is called, negatively electrified; and this, on his principles, is no doubt the true cauſe of the phenomenon.

Doctor Prieſtley ſeems to think, that the electric fluid is affected only one way, and at one end; and his reaſoning on the ſubject, and ſuch reaſons as are generally offered for the ſolution of the Leyden phenomenon, are as juſt and philoſophical as it would be to ſuppoſe that, if a certain portion were taken from, or added to, a quantity of air, the whole maſs would neither be rarer, or denſer; or when a muſical ſtring begins to return, the body of the pulſe will not be relaxed.

But, it is in vain to look for conſiſtent, and juſt reaſoning, where not only the common opinions concerning the nature of the charge are clogged with difficulties, and the principles aſſumed for the ſolution of the phenomenon are wholly arbitrary; for the very method of arguing on the ſubject, is contrary to that which is, with truth and propriety, purſued in ſimilar caſes. The method is alſo actually inconſiſtent with the avowed doctrines, and acknowledged diſcoveries of thoſe who have been the promoters, and defenders of the popular ſyſtem.

[n] What is more common than to hear of the repulſive power of excited electrics, and the conſequences of it? What more notorious than the experiment of Æpinus, in which one end of a glaſs tube being poſitively electrified, four or five inches of it were poſitive, then two inches negative, beyond which the tube was weakly poſitive again. The conſequences which flow from this experiment, are too obvious not to be ſeen, and

[n] Dr. Prieſtley's Hiſtory of Electricity, page

therefore, I ſhall not ſpend time in pointing them out, to the electrician.

[°] But we have been lately informed, that the late Mr. Henley, Signor Beccaria, and others, adopted the errors of Doctor Franklin, in ſuppoſing that the natural quantity of the electric effluvia in glaſs, could neither be increaſed nor diminiſhed; and, that it was among the impoſſible things in nature, to add any electric fluid on one ſide of a pane of glaſs, when there was no communication to lead off as much, from the other ſide to the earth.

We are now told, this is directly contrary to what really happens, and every known ſubſtance may now have its natural quantity of the electric fluid either increaſed or diminiſhed, to a certain degree; which degree bears no proportion to the extent of the ſurface. According to theſe principles, we are informed, that Doctor Franklin's reaſoning is erroneous, both in charging and diſcharging a jar; and the ſame perſon remarks, that if the diſcharge be made at once, from the poſitive to the negative ſide, there muſt be a time, when there cannot be any electric effluvia in the pores of the glaſs.

If we admit the fact, the reſult will be clear, and the difficulty muſt be ſurmounted.

[°] Philoſophical Tranſactions, part 1ſt, 1788. Doctor Grey's Paper.

But, it being impoſſible on the Franklinian theory, for glaſs to be in this ſtate, even for an inſtant, it is ſuppoſed, that both charges and diſcharges conſiſt of ſeveral ſmall, rapid, and ſucceſſive quantities of electric effluvia; which in diſcharging, paſs with an inconceivable velocity from the inſide of a jar, charged poſitively, to the outſide charged negatively.

[p] This partial attack upon the Franklinian principles, has been followed by another, in which we are told that, during the time of charging a jar, both inſide and outſide have the ſame kind of electricity; and that the negative electricity does not take place till the turning of the machine is diſcontinued. If we reflect on the foregoing experiments, and the reſults which have been drawn from them by the defenders of the Franklinian hypotheſis; they muſt be bigotted to ſyſtems indeed, if they cannot diſcern how deeply they are involved in inconſiſtencies of their own invention. I cannot find, from the foregoing obſervations, nor from any author I have yet read on this ſubject, any reaſon to give up the evidence of my ſenſes to ſupport a theory, which ought long ſince to have been exploded; and therefore, * I ſhall maintain that glaſs is permeable by the electric effluvia.

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[p] Mr. Brookes's Experiments on the Leyden phial.

* See Lyon's farther proofs, that glaſs is permeable by the electric effluvia.

CHAPTER II.

The Principles of the Electric Atmoſpheres, according to the Franklinian Syſtem, with a Brief Account of the reaſonings of different Authors on the experiments made to ſupport their Theory.

THE advocates for the Franklinian principles, having attempted to eſtabliſh the impermeability of glaſs to the electric effluvia, and their theory of poſitive and negative electricity, by the inconcluſive arguments I have already mentioned; the next difficulty was to unfold the principles, and the laws of action of electric atmoſpheres, and in this we ſhall find appearances and theory, at continual variance with each other.

As every electric phenomenon was to be explained by the ſyſtem of poſitive, and negative electricity, laws were firſt framed, and then experiments made to prove them, and by beginning at the wrong end; perhaps it will be found, that the framers of the laws have embarraſſed their labour with ſomething like inconſiſtency, and contradiction.

The Principles of Electric Atmoſpheres.

[q] 1. It is ſaid "that electric atmoſ-
"pheres, which flow round non-electric
"bodies, being brought near each other, do
"not readily mix and unite, but remain
"ſeparate, and repel each other.

2. "An electric atmoſphere not only
"repels another electric atmoſphere, but
"will alſo repel the electric matter contain-
"ed in the ſubſtance of a body approach-
"ing it; and without joining or mixing with
"it, will force it to other parts of the body.

3. "Bodies electrified either poſitively,
"or negatively, repel each other."

The inquiſitive and indefatigable Signor Beccaria, being furniſhed with theſe general principles, proceeded to unravel all the properties of electric atmoſpheres, and he publiſhed this general law, viz. [r] "That the "electricity of a body endeavours, through "the atmoſphere which it actuates, to in- "troduce a contrary electricity, into bodies "immerſed in it."

This being admitted, two conſequences naturally followed, which he called ſecondary properties of electric atmoſpheres.

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[q] Doctor Franklin's Experiments and Obſervations, page 155.

[r] Signor Beccaria's Experiments, page 180, and 186, Tranſlation.

The firſt of theſe properties is, that two bodies, either both poſitively, or both negatively electrified, and oppoſed to each other, endeavour, by means of their atmoſpheres, reciprocally to deſtroy each other. I ſuppoſe, by each exerting its power, and introducing a contrary electricity, as far as that power extends.

We might have remained contented, and without ſuſpecting the truth of this general property of electric atmoſpheres, have thought ourſelves in poſſeſſion of an invariable law, if another perſon, equally inquiſitive, had not drawn a very different concluſion from his own experiments.

[a] Signor Volta, in his method for diſcovering the weakeſt electricities, either natural or artificial, informs us, That if two flat plates of the electrophorus be electrified, both poſitively, or both negatively, and gradually brought near each other, the nearer they approach, the more their intenſities will increaſe.

If we admit the firſt ſecondary property of electric atmoſpheres to be right, according to Signor Beccaria, then the reſult, by Signor Volta, muſt appear to be wrong.

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[a] Philoſophical Tranſactions, Vol. 72, Part 1, 1782. Sect. 39.

But, when our principles are falſe, it is no wonder errors increaſe, in ſupporting them.

Signor Beccaria, in his next ſecondary property of an electric atmoſphere, ſays, "When two bodies impregnated with contrary electricities meet, theſe electricities, "by means of their intervening atmoſpheres, "reciprocally increaſe their intenſities."

This he adds is plain, for the force which endeavours to produce a contrary electricity, where none exiſts, muſt likewiſe endeavour to increaſe the ſame, when it is already obtained.

If we turn to the fortieth ſection of Signor Volta's paper before cited, we ſhall find, that when his flat covers were electrified, one poſitively, the other negatively, and were gradually brought near each other, the intenſities of their electricities were diminiſhed; becauſe their capacities were increaſed, the nearer the plates were brought to each other.

Though the two Italian philoſophers differ ſo materially, it may be expected, that more conſiſtency will be found in the Engliſh writers on this ſubject.

In a work which was profeſſedly written to fix the laws of electric attraction and repulſion, as well as the principles of electric atmoſpheres, we may reaſonably hope to find ſome fixed truths, if they are to be found in the various authors who have written in defence of the Franklinian ſyſtem.

An [t] author of high rank, in order to give his readers a clear idea of electric atmoſpheres, defines them in ſubſtance, if not in words, as follows, namely, the particles of air round bodies, electrified either poſitively, or negatively, conſtitute electric atmoſpheres; and their denſity is in ſome inverſe ratio of the diſtance from the charged body, which cauſes that electric atmoſphere.

[u] Theſe electric atmoſpheres are formed, not only by the particles of air coming into contact with the electrified body, but, in a certain degree, alſo by coming in contact with each other, and by giving, or receiving electric effluvia to or from the particles of air they touch, or approach, accordingly as the body is electrified. Theſe principles being admitted, we may venture to conclude from our experiments, how readily the particles of air (which are in the firſt claſs of electrics) part with, or receive, the electric effluvia; for with a good machine, and large conductors, there may be a poſitive, and what they call a negative atmoſphere, round them, to a conſiderable diſtance, on the firſt turn of the cylinder.

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[t] Charles Viſcount Mahon, now Earl Stanhope's Treatiſe, &c. page 7 and 8.

[u] The ſame, page 234.

But, though the air (and I will not except the drieſt) ſo readily receives the electric effluvia in ſome inſtances, we are told, it will not do it in others.

The inſtances are as follow:

[w] If the end of a metallic body, A, B. (ſee plate, fig 1.) be placed with the end A. without the ſtriking diſtance, but within what is called the ſenſible part of a negative electric atmoſphere of the conductor D, E, then from the very nature of an electric equilibrium, the electric fluid contained in the remote end of the body A, B. (we are told) muſt of neceſſity crowd towards the near end A, and endeavour to get out there; but it is prevented from leaving the inſulated body A, B, by the reſiſtance it meets with from the dry air, which is a non-conductor of electricity, and the end B will be negatively, and the end A, poſitively electrified.

When we conſider, that an inſulated electrified body readily parts with the electric effluvia to the particles of the circumambient air in their natural ſtate, and the particles of air communicate them to each other, when in contact; it certainly ſeems not a little improbable, that the particles of air, when deprived of their natural quantity of electric effluvia, ſhould not receive them from a metallic body with which they are in contact;

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[w] See Charles Viſcount Mahon's Treatiſe, page 22.

and, eſpecially when beſides it is granted, that the electric effluvia are preſſing forward, to eſcape from the end A, of[x] the body A, B.

If the end A of the inſulated body A, B, be placed within the ſenſible part of a poſitive electric atmoſphere, then the particles of the electric effluvia will be ſuperinduced upon the near end A; yet inſtead of increaſing the quantity, they will leſſen it, by repelling the natural electric effluvia in the end A, to the end B, where they will be ſtopped by the air, as in the former caſe.

Whether electric atmoſpheres act by repulſion or preſſure, as repreſented by the noble author, may be determined by an experiment.

Let A, B, fig. 2, repreſent a metallic inſulated rod, and C, D another, in contact with the rod A, B; admit at D, or any other part between A, B, and at any angle with the rod A, B.

Suſpend a pair of pith balls in the uſual manner, from the ends of the inſulated rods at A and C, and bring an excited glaſs tube near to the end B; the balls will diverge, both at A and C.

Let a perſon now put his finger upon the end of the rod C, and let the excited glaſs tube be brought again, near the end of the rod B; the effects of the ſuppoſed preſſure now ceaſe immediately, there being no divergency of the pith balls, either at A or C.

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[x] See Charles Viſcount Mahon's Treatiſe, page 24 and 25.

If the divergency of the pith balls proceed from the cauſe alleged, and the motion be propagated through an elaſtic fluid, as Sir Iſaac Newton and other writers on the ſubject have demonſtrated; what reaſon can be aſſigned, why the pith balls do not diverge at A, when the finger is at C?

The truth is, the effects produced by electric atmoſpheres, are by their communicating ſmall portions of their electric effluvia to inſulated bodies, immerſed in the ſenſible part of them; and this is, in many inſtances, acknowledged by electricians.

That an excited electric tube will communicate its electric effluvia to inſulated bodies, is evident, and demonſtrable, if there be any dependance on what we read, or what we ſee in our experiments. Mr. Wilſon informs us[y] (but then he is a ſceptic in theory, and not to be attended to) that excited glaſs applied near the end of a cylinder of wood, will communicate a quantity of its accumulated fluid to it. [z] Doctor Milner admits, that excited glaſs tubes will communicate electric effluvia, when in contact with a wire, ſufficient to charge glaſs; and that the ſphere of communication extends to ſmall diſtances, which vary, and beyond which a ſphere of influence begins.

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[y] Wilſon's Short View, page 2.

[z] Dr. Milner's Experiments, page 69 and 8.

I can myſelf add, that a phial may be charged, ſo as to give a conſiderable ſhock, when ſuſpended at the end of an inſulated metallic rod, with an excited glaſs tube, without bringing the tube cloſe to the end of the wire, by ſome inches.

If we admit (what is granted) that an excited electric, and conſequently electric atmoſpheres, act by communication when near the inſulated body, can we aſſign any rational cauſe, why they ſhould not do the ſame in every inſtance, when the body is within the ſphere of their influence?

If we admit, or can prove, as we demonſtrably can, that excited electrics will act by communication, then we muſt allow, it will be in proportion to the ſtrength of the excited electric, the form of the end of the inſulated rod, and the diſtance of it. If we depart from this rational method of deducing ſimilar effects from the ſame cauſe, and in the ſame circumſtances, we ſhall be obliged, as we have hitherto been, to amuſe ourſelves with laws and properties, which, if underſtood, are continually miſapplied, and I may ſay with J. A. De Luc,[a] Reſtons aux qualités des anciens; cela ſera plus court et tout auſſi raiſonable.

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[a] J. A. De Luc's Hiſt. de la Terre, et de l'Homme, Tom 1. page 191.

CHAPTER III.

The Direction of the Electric Effluvia ſhewn, in paſſing from the Surfaces of excited and charged Electrics.

SINCE it is univerſally allowed, that the electric effluvia paſs from the ſurface of the cylinder, to what is called the poſitive conductor, I ſhall only add at preſent, that they converge from the ſurface of the cylinder, to the point in the end of the conductor, and again diverge from another point in the oppoſite end; and, if there be no point, then in all directions from the whole ſurface of the conductor, and by the aſſiſtance of inſulated metallic rods, they may be made to converge to, and diverge from, rod to rod, till they loſe their momentum, and are abſorbed by the circumambient air.

I am ſenſible that nothing ſhort of the moſt ſtriking appearances, will prevail againſt the preſent received opinion, concerning the direction of the electric effluvia from the oppoſite ſide of the cylinder, or the cuſhion; I ſhall therefore proceed, ſtep by ſtep, with my proof.

EXPERIMENT I.

To ſhew that the Direction of the Electric Effluvia is from the back of the Inſulated Cuſhion, and not to it, as has been generally maintained.

Let a ſmall ſpiral tube (fig. 3.) twelve or fourteen inches long, be fixed in a metallic ſocket, with a wire at the end, to ſcrew into the end of the conductor C, connected with the inſulated cuſhion; and let this ſpiral tube with the ſocket, be fixed in another thin, light, glaſs tube, with the oppoſite end *a* open.

Within the end *a* of the laſt mentioned tube, and over the end of the ſpiral tube, ſlip the ſmall moveable metallic ſocket, to which is added a wire *b*, about a foot long, with a braſs knob at the end of it. This being fixed, and the other conductor ſtanding before the cylinder uninſulated; let the machine be turned briſkly, and the electric effluvia will be inſtantly ſeen in motion, not only on the ſpiral tube, but on the wire between the end of the ſpiral tube, and the braſs knob.

The ſimple and natural reſult from this experiment is, that the electric effluvia, collected from the air by the friction of the cylinder againſt the cuſhion being elaſtic, they expand above that part of the cuſhion, in contact with the cylinder, and paſs off by every conducting ſubſtance: a part is carried by the revolving of the cylinder to the point in the poſitive conductor; another portion to the conductor connected with the cuſhion; and they are ſeen in motion on the ſpiral tube; and being prevented by the braſs knob from paſſing off as faſt as they collect, are rendered viſible round the wire; and by bringing the hand, or any flat or rounded body, within three or four inches of the wire, a very denſe electric atmoſphere may be ſeen round it, in a dark room. I am ſenſible recourſe will be had to the [b] principles I have already mentioned to explain away the truth; but, if the electric effluvia run up the tube, and the electric atmoſphere be formed round the wire, by the particles of the air depoſiting their electricity as they approach it, or each other; then we muſt either admit, that the air can ſupply every part of the ſurface of the conductor connected with the cuſhion, with the electric fluid, ſo that it may run in a ſtream to the cuſhion, or we muſt grant it runs in a ſtream from it; and in either caſe, the conſequences are clear.[c] But if it be ſaid that the electric effluvia run up the ſpiral tube, I ſhould be glad to be informed, why the circumambient air does not ſupply the cuſhion by the conductor, rather than by the ſpiral tube, as it requires a conſiderable momentum to overcome the reſiſtance on the ſurface of the glaſs?

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[b] Charles Viſcount Mahon's Treatiſe, page 8, &c.

EXPERIMENT II.

With the ſame Apparatus.

Let every thing remain, as in the preceding experiment, and upon turning the cylinder, let a perſon preſent a ſharp pointed wire within four inches of the knob B, upon the ſpiral tube, and the motion of the electric effluvia will be conſiderably accelerated in their paſſage by the aſſiſtance of the point.

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[c] If the electric effluvia runs in a ſtream from the cuſhion, how are jars to be charged negatively, or, in other words, to be deprived of all their natural quantity of the electric effluvia with the ſame number of turns a jar is filled, at the poſitive conductor?

If the ſtream be toward the cuſhion, then the conducting property of the air is clear; and this will prevent bodies being negatively electrified in the ſenſe which has been maintained by the Frankliniſts.

REMARKS.

If the ſharp pointed wire ſupplies the knob B with the electric effluvia, then there ought to be a more denſe, and viſible electric atmoſphere round the wire, as there is evidently a quicker and ſtronger current on the ſpiral tube; but the viſible effects ceaſe on the wire.

But if we ſay the electric effluvia converge inviſibly to the point, and are by it conducted off, as faſt as they deſcend the ſpiral tube, and that this prevents the accumulation of the electric effluvia round the wire, we ſhall for once be right.

But again, if the ſtar be an infallible ſign that the electric effluvia are entering a point, then the direction is certainly from the cuſhion to the point. If this be denied, then the ſign has loſt its infallibility, and it exhibits the ſame appearance, in certain inſtances, whether the fluid be iſſuing or entering on the cuſhion ſide of the cylinder, which will be further proved in its proper place.

EXPERIMENT III.

Slip off the ſocket *a*, with the wire and knob *b*, from the end of the ſpiral tube (fig. 3.) and put on the concave metal cup *c*, (fig. 4.) on the end of the ſpiral tube. Let the cup be about two inches diameter, with a thin rim.

Next, let a perſon take the wire *w*, and preſent the ſmall braſs knob *d*, oppoſite to the center of the cup *c*, but beyond the ſtriking and focal diſtance of the electric rays, which appear on the edge of the cup, upon turning the cylinder. Let the diſtance of the knob be about four inches from the rim; which will be within the ſenſible part of the electric atmoſphere.

The apparatus being properly adjuſted, and the cylinder turned, the electric effluvia will be ſeen running on the ſpiral tube, and either iſſuing from, or entering the edge of the cup *c*, without the leaſt inclination to the knob.

REMARKS.

According to the principles mentioned, page 23 of the before cited noble author, the effluvia cannot proceed from the knob to the edge of the cup, for they are prevented by the dry air; notwithſtanding they are preſſing forward to get off. As they cannot iſſue from the knob, I am certain they do not converge to it from the edge of the cup; for they diverge from thence into the circumambient air, in the ſame manner as if the ball was not within the ſenſible part of the

the electric atmoſphere of the cup; neither are there any more than the direct rays iſſuing from the concave part which reach the knob, and theſe enter it. But, if the directions of the electric effluvia be denied to be from the cuſhion, then it muſt be granted, that there is a current of the electric effluvia ſetting in from the circumambient air, as mentioned in the remarks on the firſt experiment; and in either caſe, I would aſk any unprejudiced perſon, how he is to deprive a jar of all its natural electric fluid, in the ſame time he can fill another of the ſame dimenſions, at the poſitive conductor, when there is a rapid ſtream ſetting in during the whole operation?

Let the perſon holding the braſs knob, turn the end *e* of the wire, with a ſharp point, and hold it as he held the knob, oppoſite to the center of the cup *c*, and at the ſame diſtance; then upon turning the cylinder, the motion of electric effluvia will be accelerated on the ſpiral tube, and they will be alſo ſeen on the edge of the cup *c*, and there will be a ſtar on the point of the wire.

REMAKRS.

By bearing in mind what has been ſaid of [c] electric atmoſpheres, and the manner of their acting on bodies immerſed within the ſphere of their influence, the point will be poſitively electrified; and the electric effluvia will not only be crowding to it from the hand of the perſon holding it, but iſſuing from the point, if we can depend on the foregoing principles and properties of electric atmoſpheres. On the ſame principles the current from the point ought to prevent the appearance of the electric effluvia on the edge of the cup, and alſo the negative atmoſphere, as it is called; which may be proved to extend to a conſiderable diſtance on each ſide of the poſitively electrified point. I therefore conclude that the direction of the electric effluvia is from the cuſhion, and diverging in all directions from the edge of the cup *c*; but ſome of the direct rays, iſſuing from the concave part of the cup, and ſuch others as are paſſing near the point of the wire, converge to it, and cauſe the bright appearance of a ſtar upon entering; while the more remote rays paſs off diverging, and cauſe the luminous appearance on the rim of the cup, and the electric atmoſphere, as mentioned above.

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[c] Charles Viſcount Mahon's Treatiſe, page 22, &c.

If the direction of the electric effluvia from the cuſhion be denied, then it muſt be granted, that the ſtar is the ſign that the electric effluvia are iſſuing from a point. It muſt alſo be admitted from the appearance, on the edge of the cup, and the atmoſphere round it, that it is receiving the electric effluvia from the circumambient air, as well as from the point; and if this be granted to ſupport a theory, which I have for ſome years thought never had any foundation in truth, then the experiments which have been offered by the Frankliniſts, and the reaſonings and the reſults deduced from them, to explain the properties of electric atmoſpheres, prove nothing.

I cannot conceive how it can be poſſible for any one to explain the appearances I have mentioned, by what Mr. Nicholſon calls the returning fire; as I look upon his experiments to be a direct proof of what I have offered, though he has drawn a very different reſult from them; I ſhall therefore very briefly make my remarks on his obſervations, ſo far as they relate to the point in queſtion.

[d] If the cuſhion ſerves only to ſupply the electricity at the line of contact with the cylinder, and to receive it back again at the ſeparating line; how was the cylinder ſupplied with the fluid, when it was an inch from it? Was there then an under, or an upper current? No, the excitation was performed by the ſilk only; which is, he informs us, the chief agent in excitation.

If the friction of the cylinder againſt the ſilk collects the electric effluvia, does not this prove, as clearly as an experiment can do it, that the electric fluid is univerſally diffuſed? and when glaſs is rubbed, which is a proper exciter, the electric effluvia which are immediately adjacent to the ſurface, are put in motion, and move towards it, and a proportionate motion is produced in the circumjacent effluvia, with a tendency to the ſame exciter, while the friction is continued.

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[d] Philoſophical Tranſactions, 1789, part 2d. page 265. Mr. Nicholſon's Experiments and Obſervations on Electricity.

Can any one doubt, as to the receſs of the fluid from the cylinder, who has any knowledge of the expanſion of elaſtic bodies, and with what force they expand? Is it not known, that they will force a paſſage where there is the leaſt reſiſtance, and, where is this, but in the two conductors?

If I was ſo diſpoſed, I could eaſily produce a ſeries of experiments, to ſhew what confuſion muſt enſue, by admitting two currents to be in motion in contrary directions from the cuſhion to the cylinder; but I have too high an opinion of Mr. Nicholſon's candour and abilities, to think there is any occaſion for it. I therefore haſten to the fifth experiment.

To ſhew the Divergency of the Electric Effluvia from the Conductor connected with the Cuſhion.

FIX on the end of the conductor, fig 5, connected with the cuſhion, a large blunt-pointed wire *m*: and round the end *n*, ſtick a thin piece of the beſt Dutch ſealing-wax; let the ſealing-wax be lighted, and while it is burning, let the cylinder be turned briſkly, and the wax, while in a melting ſtate, will be carried by the electric effluvia, diverging from the end of the wire in inconceivably ſmall rays, to the diſtance of ſeveral feet.

REMARKS.

Theſe waxen diverging rays, ſhew us the direction of the inviſible diverging rays of the electric effluvia, from what has been called a negative conductor. The ſame arguments may be uſed which I have already mentioned, to ſhew that the electric effluvia diverge inviſibly, after they leave a point in a conductor connected with the cuſhion; and ſeveral experiments made with the rays of ſealing-wax might be adduced; but I rather chuſe to offer the following propoſition, and proceed immediately to prove it.

PROPOSITION.

The electric effluvia, in paſſing from the ſurface of excited electrics, diverge in all directions, where there are not any conducting ſubſtances to receive them; but, where there are metallic conductors placed near the rubbed cylinder, and the inſulated rubber, they tend to each conductor, though in contrary directions, and form different appearances, at ſmall diſtances through the air, according to the form of the ſurfaces they leave, and thoſe they approach.

AS electricians all appear to be agreed concerning the direction of the electric effluvia on the poſitive ſide of the cylinder, experiments will be uſeleſs to ſhew what all admit; but in order to ſhew the different appearances of the electric effluvia in their paſſage from the inſulated cuſhion, from body to body, according to the ſurfaces they leave, and approach, it will be neceſſary to produce the proofs, as the reſults drawn from experiments amongſt electricians, are frequently different, when the evidence produced to the ſenſes is the ſame.

EXPERI-

EXPERIMENT I.

To ſhew the Electric Effluvia converging from the Back of the inſulated Cuſhion, to a blunt pointed Wire, in the end of the Conductor.

Screw a blunt pointed wire, *p* (fig. 6) into the end of the conductor, on the cuſhion ſide of the cylinder, and a ſharp pointed wire, *q*, into the other end, fartheſt from the cuſhion; place the conductor at right angles with the back of the cuſhion, and let the blunt pointed wire *p*, be about an inch and a half from it. Upon turning the cylinder, a pencil of rays will be ſeen between the back of the cuſhion, and the point of the blunted wire, and a ſtar at the point of the ſharp wire *q*.

EXPERIMENT II.

Or the Firſt varied.

Take out the blunt pointed wire *p*, and ſcrew a very ſharp one in its place, and let the conductor remain in the ſame poſition, with the point about two inches from the back of the cuſhion, and upon turning the cylinder, a ſtar will appear on both points, *p* and *q*.

R E M A R K S.

As the appearance of the electric effluvia may be varied at pleaſure, by only altering the form and the diſtance of the metallic ſubſtances, it is plain that the ſigns of the ſtar, and the bruſh, are no certain indications, that the electric effluvia are either iſſuing from, or entering into points, as has hitherto been ſuppoſed.[e]

Inſtead of offering any farther remarks on the foregoing experiments, to ſhew the conſequences which muſt follow by reaſoning on the ſtars at each end of the conductor, I ſhall produce the teſtimony of a rigid Frankliniſt to ſhew, that the direction of the electric effluvia is from what he calls a negative electrified conductor, although he denies in theory, what the evidence of his ſenſes has extorted from him in practice.

[f] He informs us, that *a ſpark drawn from a conductor in a negative ſtate*, is much more pungent than a ſpark drawn from a ſimilar, and equal conductor, in an equal *plus* ſtate.

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[e] Cavallo's Complete Treatiſe, page 207.

[f] Mr. Nairn's Treatiſe, page 45 and 48.

The cauſe of this ſingular phenomenon, he ſays, is as follows; "the ſpark proceeding "from the plus conductor, is emitted from a "ſingle point of the ſurface; but when it "has proceeded about one third of its length, "it becomes divided into many radiations, "ſpringing from a kind of luminous ſpeck; "it does not therefore enter the hand of the "obſerver at one, but at many points of the "ſurface, conſequently its effects are divided, "and weakened. But the contrary happens, "when the conductor is in a negative ſtate, "*the ends of the ſpark being, as it were, reſerved*. The paſſage of the electric effluvia is made through a ſingle point, or "ſmall part of the ſkin of the obſerver, and "the irritation becomes greater."

EXPERIMENT III.

Connect the conductor *n*, (fig. 7.) with the cuſhion, ſo that there be a communication between them, by ſome metallic ſubſtance. Upon the end of the conductor fartheſt from the cuſhion, ſcrew on the metallic cup *c*, mentioned in the preceding experiments, figure 4, and about two inches in diameter.

Upon the glaſs ſtand *g*, let there be fixed a moveable ſocket (1) to ſupport a glaſs tube (2) fixed in the horizontal ſocket (3) with a cap (4) at the end, to ſupport a perpendicular wire (5) with a blunted point at the lower end, and a flat metal plate (6) about the ſize of the cup, and fixed upon its center on the other end of the wire (5).

The perpendicular wire (5) is to be brought with the lower end over the center of the concave cup (*c*) and by the moveable ſocket (1) fixed with the end about an inch and a half above it.

Another wire (7) may then be held perpendicularly over the center of the metallic plate (6) with a blunted point, and about an inch and a half from it.

If the cylinder be then turned, the cup *c* being concave, a hollow cone of rays will converge from the rim to the point of the wire (5) and from every part of the ſurface of the plate (6) a converging pencil of rays will riſe to the wire (7) and the apex of each of the cones will be at the points of the wires, 5, 7.

Raiſe the points of the wires (5 and 7) a little above the focal diſtances of each pencil, and inſtead of two regular and well formed pencils of rays, there will appear at each point, a very imperfect pencil of rays, without any apparent baſe; and in converging to the points, the rays which form the apex of each pencil, will be one longer than another. This is the natural conſequence of raiſing the points beyond the focal diſtance of each pencil, for then the electric effluvia diverge off from the edges of the cup and plate in every direction, and many of the extreme inviſible rays will be too far off to converge to the points; neither can any more than ſuch rays as paſs near the points, converge to them.

Raiſe the points between three and four inches above the ſurfaces of the cup and plate, and there will be only a ſtar at each point; and in this caſe, the few rays which converge to the points, are too rare to cauſe any other appearance.

Raiſe the points a little higher, and the diverging rays will be too rare to cauſe any luminous appearance at the points, and they will only be ſeen iſſuing from the edges of the cup and plate, in very ſhort rays, which form an inviſible electric atmoſphere, when they have no direct tendency to any conducting body.

Theſe experiments may be varied, and ſtars, and bruſhes, as they have been called, may be produced at pleaſure, to prove or contradict any thing, by only changing the ſurfaces from which the effluvia converge to differently pointed wires, held nearer to, or farther from, the ſurfaces. But amidſt this variety, there is one general, and invariable law, which is, that the electric effluvia will converge in a viſible well formed pencil of rays, from a broad ſurface connected with the cuſhion, if the point be not beyond the focal diſtance; and if beyond this, and ſharp, they will converge inviſibly; and they who cannot admit that the electric effluvia paſs from the cuſhion, inſtead of going to it, from the experiments and the remarks produced, will, I make no doubt, reſt ſatisfied with the preſent perplexed ſtate of the popular theory, founded upon conjecture, and ſupported by the apparent contradictions of its defenders.

The action of points and knobs, and all other electric phænomena, both natural and artificial, may be explained by a few very ſimple principles; but this will lead me beyond the limits of my preſent deſign, which is only briefly to point out the confuſion and contradiction which have been introduced, in defending the preſent received theory of electricity; and to point out the direction of the electric effluvia from the inſulated cuſhion, and in doing this, I conſider myſelf as having included the direction of the electric effluvia from the ſurfaces of charged electrics.

FINIS.

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